**Peer Review and Communication History**

**MS Title**: Garbage in, Garbage Out?

Evaluating the Evidentiary Value of Published Meta-analyses Using Z-Curve Analysis

**Author Name**: Lukas K. Sotola

**Submitted:** Oct 26, 2021

**Editor First Decision**: Revise & Resubmit

Dec 21, 2021

Dear Lukas K. Sotola,

I have now received all reviews of your manuscript, “Garbage in, Garbage Out? Evaluating the Evidentiary Value of Published Meta-analyses Using Z-Curve Analyses” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology.

The reviewers, both experts in Z-curve, did an outstanding job in their reviews. So much so that I am in the unusual position to have nothing else to add. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Chris Aberson

Reviewer 1

# Reviewer 1

##### Open response questions

### Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

The authors assess evidence of 25 meta-analyses published in the Psychological Bulletin within the last 10 years. The authors use z-curve, a novel alternative to p-curve that is capable of incorporating heterogeneity of effect sizes into account, to estimate different indices evidence such as expected replication rate, expected discovery rate, file-drawer ration and etc. The authors find that majority of the meta-analyses do not contain adequate evidence with z-curve estimates suggestive of publication bias and QRP. The authors end with a cautionary note on interpreting results from meta-analyses.
I generally like the manuscript. The authors address a relevant issue – the reliability and trustworthiness of the published research record and they do it with the state-of-the-art techniques. Nevertheless, I identified few major and minor points that the authors should address before I can recommend publication of the manuscript.
Major points:

* Please consider including a diagram depicting the data gathering process (i.e., the number of considered meta-analyses, the number of meta-analyses excluded due to different criteria etc…).
* Please clarify the differences from the linked pre-registration document. E.g., excluding meta-analyses with less than 20 studies (pre-registration) vs excluding meta-analyses with less than 10 studies (manuscript).
* I strongly advice the authors to use bootstrap and report confidence intervals of each of the estimates. Simulations studies showed that especially the EDR estimates are noisy in small sample sizes.
* p. 5: In recent years, two statistical methods, p-curve analysis and z-curve analysis (Brunner & Schimmack, 2020; Simonsohn et al., 2014), have been developed to help estimate the degree to which researchers in an area may have engaged in QRPs
While p-curve provides an explicit test of p-hacking (e.g., testing for left-skew in the case of intense p-hacking), both methods correspond to a type of selection model; p-curve to a fixed effect selection model ignoring all statistically non-significant studies and z-curve to a selection model with heterogenous studies (modeled via a mixture distribution on studies’ power) ignoring statistically non-significant studies. That means that the estimates (power, ERR, and EDR) assume that significant results were reported reliably — while many cases of p-hacking correspond to this assumption ( selection for statistical significance), e.g., outcome switching and dropping studies, other do not, e.g., adding participants and removing outliers until reaching significance. Consequently, the p-curve and z-curve estimates might be biased in such scenarios. I would, therefore, suggest not signifying the QRP to such a degree. I would instead suggest framing the arguments in terms of reliability of the literature, which can encompass both the publication bias and some types of QRP.
* p. 6: Both p-curve and z-curve analysis capitalize on the ways in which QRPs affect the distribution of p-values from published studies. They both work backwards from this distribution, comparing the observed and expected distribution of p-values to determine the prevalence of QRPs, and to estimate publication bias and replicability for a set of published study.
This is not technically correct as z-curve is estimated with the distribution of z-statistics. (And similarly to the previous comment, both model selection for statistical significance, not p-hacking directly.)

Minor points:

* Italicize test statistics (d) and p-values throughout the manuscript (I appreciate the irony of not italicizing them in my review; however, the review submission portal removed all of my formattings.)
* p. 11: Z-curve 2.0 was used for the main analysis (Bartoš & Schimmack, 2020). This analysis takes the p-values from a set of studies of interest to the researcher, converts them to one-tailed z-scores, and uses those z-scores to calculate average power of all of the studies that have hypothetically been done using finite mixture modeling.
This is in-fact incorrect. Z-curve is estimated using two-sided z-statistics, via a mixture of truncated folded normal distributions (only the ERR estimate is based on extrapolation taking directionality of the tests into account).
* Soric FDR, file-drawer ratio, and the number of missing studies are all transformations of EDR. While each of them provides a different way of looking at the results, it should be noted that they theoretically present the same information.
* p. 13: The analysis in R will only run if there are at least 10 p-values below .05 in the data entered, because if there are too few significant p-values in the z-curve, then the estimates may be highly inaccurate.
The R-code limitation for 10 studies is more of a sanity check, not the recommended minimum sample size. Simulations by Bartos & Schimmack (2020) did not consider lower than 100 p-values (note that behavior of p-curve is even worse in small sizes). As noted in the major points, present CI so readers can assess precision of the estimates (they will be highly variable in many cases, which is an inherent limitation of extrapolating from a limit data).

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  | ✔ |  |

# Reviewer 2

##### Open response questions

### Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

As the co-inventor of z-curve, I am not an unbiased neutral observer. However, I can comment on the scientific contribution of this manuscript. The strength of this manuscript is to apply the novel z-curve method to several highly influential meta-analyses. The results show how meta-analysts can benefit from z-curve analyses to examine not just the presence of publication bias (QRPs/selection bias), but also quantify the extent of publication bias. In addition, meta-analysts can provide important information about the average power of past studies, which can guide sample size decisions in future studies. The results for some meta-analyses are stunning and alarming (e.g. Weingartner’s meta-analysis of priming) and show that published meta-analyses in highly cited journals cannot be trusted. I think these results will stimulate more rigorous examinations of publication bias in the future.
The main limitation of z-curve is that it does not correct effect size estimates for publication bias. Effect sizes are typically the main focus of meta-analysis. It might be interesting to add results of bias-detection and bias-correction methods for effect sizes, but I do not think this is necessary for publication. It could also be a future project.

Overall, this is a well-designed and well-executed project with clear results that can advance the use of meta-analysis in psychology. Below are a few detailed comments that may increase clarity of the ms.

Best, Ulrich Schimmack

Detailed comments

“In recent years, two statistical methods, p-curve analysis and z-curve analysis (Brunner & Schimmack, 2020; Simonsohn et al., 2014), have been developed to help estimate the degree to which researchers in an area may have engaged in QRPs”
This is not correct. P-curve has been advocated as a tool to examine whether published studies have evidential value (i.e., the null-hypothesis that all significant results are false positives can be rejected). The p-curve authors themselves make clear that p-curve does not test for the presence of QRPs or selection bias. The ability to examine publication bias would be a good reason to use z-curve, but even the authors’ stated reason that z-curve provides more accurate estimates of mean (unconditional) power after selection for significance than p-curve (i.e., the expected replication rate) is good enough.

I personally prefer FDR as percentages as I think most people find it easier to think in terms of percentages than in terms of ratios (20% versus 1 over 5), but that is just a personal comment.
“Thus, for example, an FDR of 15.93 suggests that for every published study, there are predicted to be 15.93 unpublished studies. So the higher the FDR, the greater the possibility for the existence of publication bias” I don’t think that the file drawer ratio (FDR) is needed in addition to information about the EDR. In fact, it is not really a measure of the file drawer if non-significant results are reported. Therefore, I suggest to focus on ODR – EDR and to delete FDR results.

Figure 1 - I would find it more intuitive to have high positive numbers to reflect the largest amount of publication bias (ODR – EDR = Publication Bias).

Figure 2 again, I would prefer percentages Weingarten 15:1 would imply (15/16 = .94) 94% False Discovery Risk !!! That is a stunning finding.

Weakest evidence for an effect that can be compared to p-curve results would be the replicability estimate that corresponds directly to the power estimate in p-curve (i.e. both are measures of unconditional mean power after selection for significance).

The author/s correctly note that p-curve power estimates should match z-curve ERR estimates. This is the case when power is low as in the Weingartner meta-analysis because low power means homogeneous effect sizes. However, Tannenbaum’s p-curve estimate of 91% does not match the z-curve estimate of 53.6%. The reason is that p-curve is sensitive to heterogeneity and the presence of a few studies with strong evidence. Even the p-curve authors are aware of this bias (<http://datacolada.org/67>). They just prefer to ignore this issue. Here we see that we would come to very different conclusions about the studies in the Tannenbaum meta-analysis based on biased p-curve estimates and unbiased z-curve estimates. Another good reason to use z-curve for this project. The same applies to Fischer (84% for p-curve; 63.6% for z-curve).

##### Rating scale questions

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  |  | ✔ |

**Author Response**
Jan 28, 2022

Dear XXXXXX,

I have now received all reviews of your manuscript, “Garbage in, Garbage Out? Evaluating the Evidentiary Value of Published Meta-analyses Using Z-Curve Analyses” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology.

The reviewers, both experts in Z-curve, did an outstanding job in their reviews. So much so that I am in the unusual position to have nothing else to add. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

Thank you very much for your generous comments. I have double-checked that the manuscript follows *Collabra: Psychology*’s submission guidelines and double-checked the image files for the figures. You will notice that I have removed one figure, a change which I explain in one of my responses to a comment from Reviewer 1. I have also added the references for each meta-analysis included in my analysis to the References.

If you have any questions or difficulties during this process, please contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Chris Aberson

Reviewer 1

**Reviewer 1**

The authors assess evidence of 25 meta-analyses published in the Psychological Bulletin within the last 10 years. The authors use z-curve, a novel alternative to p-curve that is capable of incorporating heterogeneity of effect sizes into account, to estimate different indices evidence such as expected replication rate, expected discovery rate, file-drawer ration and etc. The authors find that majority of the meta-analyses do not contain adequate evidence with z-curve estimates suggestive of publication bias and QRP. The authors end with a cautionary note on interpreting results from meta-analyses.
I generally like the manuscript. The authors address a relevant issue – the reliability and trustworthiness of the published research record and they do it with the state-of-the-art techniques. Nevertheless, I identified few major and minor points that the authors should address before I can recommend publication of the manuscript.

Major points:

* Please consider including a diagram depicting the data gathering process (i.e., the number of considered meta-analyses, the number of meta-analyses excluded due to different criteria etc…).

I have added a paragraph to the Inclusion Criteria subsection within the Method with this information. A full PRISMA diagram seems excessive, especially with the number of figures in the paper already.

* Please clarify the differences from the linked pre-registration document. E.g., excluding meta-analyses with less than 20 studies (pre-registration) vs excluding meta-analyses with less than 10 studies (manuscript).

A new section has been added to the beginning of the Method section indicating and justifying changes from the pre-registered Method. The 10 studies/20 studies inclusion criterion was a mistake on my part in the first submission. Thank you for catching it. I have altered the text to indicate the correct inclusion criterion: that a meta-analysis had to have 20 or more studies to be included. I also realized while taking care of all of this that I forgot to upload my disclosure table on the coded meta-analyses to the Open Science Framework. I have now done that.

* I strongly advice the authors to use bootstrap and report confidence intervals of each of the estimates. Simulations studies showed that especially the EDR estimates are noisy in small sample sizes.

This change has been made. However, adding in confidence intervals made Table 1 too large to fit into the paper, so the Missing N and N columns have been removed. They seem to contain information which readers will be less interested in and can seek out on the OSF. I have left in the Significant N column so that readers can get an idea of how large each sample of p-values analyzed was.

* *p. 5: In recent years, two statistical methods, p-curve analysis and z-curve analysis (Brunner & Schimmack, 2020; Simonsohn et al., 2014), have been developed to help estimate the degree to which researchers in an area may have engaged in QRPs*
While p-curve provides an explicit test of p-hacking (e.g., testing for left-skew in the case of intense p-hacking), both methods correspond to a type of selection model; p-curve to a fixed effect selection model ignoring all statistically non-significant studies and z-curve to a selection model with heterogenous studies (modeled via a mixture distribution on studies’ power) ignoring statistically non-significant studies. That means that the estimates (power, ERR, and EDR) assume that significant results were reported reliably — while many cases of p-hacking correspond to this assumption ( selection for statistical significance), e.g., outcome switching and dropping studies, other do not, e.g., adding participants and removing outliers until reaching significance. Consequently, the p-curve and z-curve estimates might be biased in such scenarios. I would, therefore, suggest not signifying the QRP to such a degree. I would instead suggest framing the arguments in terms of reliability of the literature, which can encompass both the publication bias and some types of QRP.

I have revised the Introduction in an attempt to put less emphasis on QRPs by also discussing the replicability of the primary studies included in meta-analyses. I have also added a few clarifying statements pointing out that it is p-hacking, specifically, and not all QRPs, that z-curve/p-curve can estimate to a degree, while they may be unable to pick up instances of QRPs where statistically significant results are not reported reliably.

* *p. 6: Both p-curve and z-curve analysis capitalize on the ways in which QRPs affect the distribution of p-values from published studies. They both work backwards from this distribution, comparing the observed and expected distribution of p-values to determine the prevalence of QRPs, and to estimate publication bias and replicability for a set of published study.*
This is not technically correct as z-curve is estimated with the distribution of z-statistics. (And similarly to the previous comment, both model selection for statistical significance, not p-hacking directly.)

I have added a clarifying statement to this passage, pointing out that the major difference between p-curve and z-curve is that for z-curve, the p-values are converted to two-sided z-scores.

Minor points:

* Italicize test statistics (d) and p-values throughout the manuscript (I appreciate the irony of not italicizing them in my review; however, the review submission portal removed all of my formattings.)

I have seen to this. Thank you for pointing that out.

* *p. 11: Z-curve 2.0 was used for the main analysis (Bartoš & Schimmack, 2020). This analysis takes the p-values from a set of studies of interest to the researcher, converts them to one-tailed z-scores, and uses those z-scores to calculate average power of all of the studies that have hypothetically been done using finite mixture modeling.*
This is in-fact incorrect. Z-curve is estimated using two-sided z-statistics, via a mixture of truncated folded normal distributions (only the ERR estimate is based on extrapolation taking directionality of the tests into account).

Thank you for this correction. I have altered passages where I refer to the z-statistics to say that they are two-tailed.

* Soric FDR, file-drawer ratio, and the number of missing studies are all transformations of EDR. While each of them provides a different way of looking at the results, it should be noted that they theoretically present the same information.

I have added a note to the end of the Analytic Plan pointing this out.

* *p. 13: The analysis in R will only run if there are at least 10 p-values below .05 in the data entered, because if there are too few significant p-values in the z-curve, then the estimates may be highly inaccurate*.
The R-code limitation for 10 studies is more of a sanity check, not the recommended minimum sample size. Simulations by Bartos & Schimmack (2020) did not consider lower than 100 p-values (note that behavior of p-curve is even worse in small sizes). As noted in the major points, present CI so readers can assess precision of the estimates (they will be highly variable in many cases, which is an inherent limitation of extrapolating from a limit data).

I have added confidence intervals to Table 1. Thank you.

**Reviewer 2**

As the co-inventor of z-curve, I am not an unbiased neutral observer. However, I can comment on the scientific contribution of this manuscript. The strength of this manuscript is to apply the novel z-curve method to several highly influential meta-analyses. The results show how meta-analysts can benefit from z-curve analyses to examine not just the presence of publication bias (QRPs/selection bias), but also quantify the extent of publication bias. In addition, meta-analysts can provide important information about the average power of past studies, which can guide sample size decisions in future studies. The results for some meta-analyses are stunning and alarming (e.g. Weingartner’s meta-analysis of priming) and show that published meta-analyses in highly cited journals cannot be trusted. I think these results will stimulate more rigorous examinations of publication bias in the future.
The main limitation of z-curve is that it does not correct effect size estimates for publication bias. Effect sizes are typically the main focus of meta-analysis. It might be interesting to add results of bias-detection and bias-correction methods for effect sizes, but I do not think this is necessary for publication. It could also be a future project.

Thank you for this suggestion. I have elected not to do it for this project, as I think the results as they currently stand say enough on their own. As you say, this could be a follow-up project.

Overall, this is a well-designed and well-executed project with clear results that can advance the use of meta-analysis in psychology. Below are a few detailed comments that may increase clarity of the ms.

Best, Ulrich Schimmack

Detailed comments

“In recent years, two statistical methods, p-curve analysis and z-curve analysis (Brunner & Schimmack, 2020; Simonsohn et al., 2014), have been developed to help estimate the degree to which researchers in an area may have engaged in QRPs”
This is not correct. P-curve has been advocated as a tool to examine whether published studies have evidential value (i.e., the null-hypothesis that all significant results are false positives can be rejected). The p-curve authors themselves make clear that p-curve does not test for the presence of QRPs or selection bias. The ability to examine publication bias would be a good reason to use z-curve, but even the authors’ stated reason that z-curve provides more accurate estimates of mean (unconditional) power after selection for significance than p-curve (i.e., the expected replication rate) is good enough.

In their original paper, Simonsohn et al. (2014) write that p-curve is supposed to test for (among other things) the presence of p-hacking in a set of studies. They spend a good amount of their paper describing why p-hacking would bias the distribution of p-values and how p-curve can detect p-hacking. For example: “The practices of p-hacking and the file-drawer problem mean that a statistically significant finding may reflect selective reporting rather than a true effect. In this article, we introduce p-curve as a way to distinguish between selective reporting and truth. P-curve is the distribution of statistically significant p values for a set of independent findings. Its shape is diagnostic of the evidential value of that set of findings. We say that a set of significant findings contains evidential value when we can rule out selective reporting as the sole explanation of those findings” (p. 535). I would also cite what Reviewer 1 wrote, confirming that p-curve explicitly tests for the presence of p-hacking, at least via a test for selective reporting of significant results.

I personally prefer FDR as percentages as I think most people find it easier to think in terms of percentages than in terms of ratios (20% versus 1 over 5), but that is just a personal comment.
“Thus, for example, an FDR of 15.93 suggests that for every published study, there are predicted to be 15.93 unpublished studies. So the higher the FDR, the greater the possibility for the existence of publication bias” I don’t think that the file drawer ratio (FDR) is needed in addition to information about the EDR. In fact, it is not really a measure of the file drawer if non-significant results are reported. Therefore, I suggest to focus on ODR – EDR and to delete FDR results.

I would like to provide readers with as much information as they might want (within reason). I have revised the paper to emphasize the EDR more and changed the EDR – ODR to the ODR – EDR, but I do not think it is necessary to leave out everything else. Readers will presumably seek out what they find most interesting, and hopefully emphasizing the EDR and the ODR – EDR will lead them there. I have also converted the FDR to percentages, as you recommended.

Figure 1 - I would find it more intuitive to have high positive numbers to reflect the largest amount of publication bias (ODR – EDR = Publication Bias).

I have changed the EDR – ODR to the ODR – EDR.

Figure 2 again, I would prefer percentages Weingarten 15:1 would imply (15/16 = .94) 94% False Discovery Risk !!! That is a stunning finding.

I have made this alteration throughout the paper. Thank you.

Weakest evidence for an effect that can be compared to p-curve results would be the replicability estimate that corresponds directly to the power estimate in p-curve (i.e. both are measures of unconditional mean power after selection for significance).

I do not know if I am meant to revise something on the basis of this comment. However, I think, based on the comments made below, I made this clear in the manuscript.

The author/s correctly note that p-curve power estimates should match z-curve ERR estimates. This is the case when power is low as in the Weingartner meta-analysis because low power means homogeneous effect sizes. However, Tannenbaum’s p-curve estimate of 91% does not match the z-curve estimate of 53.6%. The reason is that p-curve is sensitive to heterogeneity and the presence of a few studies with strong evidence. Even the p-curve authors are aware of this bias (<http://datacolada.org/67>). They just prefer to ignore this issue. Here we see that we would come to very different conclusions about the studies in the Tannenbaum meta-analysis based on biased p-curve estimates and unbiased z-curve estimates. Another good reason to use z-curve for this project. The same applies to Fischer (84% for p-curve; 63.6% for z-curve).

I have added a note about this to the end of the Results section. Thank you for pointing it out.

**Editor Final Decision: Accept**

Feb 8, 2022

Dear Lukas K. Sotola,

I have now had a chance to read over your manuscript “Garbage in, Garbage Out? Evaluating the Evidentiary Value of Published Meta-analyses Using Z-Curve Analysis”, along with the letter describing the changes you made. Thank you for your responsiveness to the concerns that the reviewers and I raised. I am happy to say that your paper is now officially accepted for publication in Collabra: Psychology. Congratulations on this excellent work, I think it will make an important contribution to the literature and I look forward to seeing it published! I hope your experiences with Collabra: Psychology have been positive and that you will continue to consider it as an outlet for your work.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point.

You will be receiving separate correspondence regarding any production and technical comments, data deposits, as well as publication charges. We work with the Copyright Clearance Center to process any applicable APC charges. Please note that your APC transaction must be completed before your article gets published.

You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely,
Chris Aberson